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Proceedings of the Inner Bay of Fundy Salmon Working Group Regional Advisory Process Maritimes Region

September 27, 1999

E. Kenchington, Chairman

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October 1, 1999

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ABSTRACT

The Inner Bay of Fundy Salmon Working Group was created to provide scientific review to a status report on Atlantic Salmon prepared for submission to the Committee on Status of Endangered Wildlife in Canada (COSEWIC). The initial review of the manuscript was conducted electronically, with members of the review panel submitting detailed comments. Those comments were incorporated into a second draft manuscript that was circulated to the review panel. A conference call between the review panel and the author was placed to discuss the changes to the manuscript and to reach a consensus on the conclusions presented therein. The final document produced was in the form of a COSEWIC Status Report.

RÉSUMÉ

Le Groupe de travail sur le saumon de l'arrière-baie de Fundy a été créé pour procéder à l'examen scientifique d'un rapport sur l'état des stocks de saumon de l'Atlantique destiné au Comité sur le statut des espèces menacées de disparition au Canada (CSEMDC). L'examen initial du manuscrit a été effectué par voie électronique et les examinateurs ont présenté des commentaires détaillés. Ceux-ci ont été intégrés à une deuxième ébauche du document, qui a été distribuée aux examinateurs. Lors d'une conférence téléphonique, ces derniers et l'auteur ont discuté des changements à apporter au manuscrit et sont parvenus à un consensus sur les conclusions qui y sont présentées. Le document définitif a été produit sous forme de Rapport sur l'état des stocks du CSEMDC.

INTRODUCTION

A small review panel under RAP, entitled the Inner Bay of Fundy Salmon Working Group, was struck on August 18, 1999 to review the technical details of a document prepared for submission to the Committee on Status of Endangered Wildlife in Canada (COSEWIC) by Peter Amiro, Science Branch, DFO, Dartmouth, Nova Scotia. This submission was seen as an action item arising from the RAP assessments of this stock assemblage (DFO Science, 1999; Marshall et al., 1999). Management considerations contained in these reports strongly advocate special measures to prevent their extirpation if the stocks are to persist (DFO Science, 1999; Marshall et al., 1999). The status of these stocks and the accuracy and appropriateness of the analytical methods used to determine that status are reviewed annually through the stock assessment process. The intent of the present review was only to ensure that the COSEWIC submission prepared by Mr. Amiro was scientifically accurate and that evidence in support of the recommendtion was presented without bias. The RAP Co-ordinator identified two key issues to consider: 1) the distinctness of the inner Bay of Fundy salmon and 2) the status of the resource using the COSEWIC guidelines. Ellen Kenchington, Oceans Branch, DFO Maritimes Region was asked to chair the meeting with assistance from John Ritter, Science Branch, DFO Maritimes Region. The chair was charged with assembling a review panel of experts and conducting a thorough scientific review of the COSEWIC submission.

Reviewers were identified and contacted to ascertain their willingness to participate in the process. Six of the seven reviewers approached agreed to participate (Appendix 1). Comments were also accepted from four other participants (Appendix 1), although they were not considered as members of the review panel. The reviewers comprised DFO scientists from both the Atlantic and Pacific coasts, a fisheries genetics consultant and university professor, a member of the New Brunswick provincial government, and a member of the Atlantic Salmon Federation. With a covering letter (Appendix 2) including the remit (Appendix 3) all reviewers were sent a copy of the draft submission prepared by Mr. Amiro (Appendix 4) as well as a copy of the COSEWIC guidelines. This information package was sent both electronically and by regular post on August 24, 1999. Reviewers were asked to provide written comments (Appendix 5) by September 10, 1999.

All comments received from the reviewers, as well as those provided by other participants (Appendix 5) were given to Mr. Amiro. Only one reviewer did not provide written comments (Dr. R. Bradford) but instead consulted verbally with Mr. Amiro. The draft manuscript was rewritten in light of those comments and a new revised manuscript was prepared. The comments in Appendix 5 relate to the original draft manuscript. All reviewers were sent a copy of the revised manuscript on September 24, 1999 and a conference call was arranged with the author (P. Amiro) participating for September 27th.

Two reviewers were not able to attend the conference call (Drs. A. Boer and R. Saunders) due to previous commitments but relayed their satisfaction with the changes to the document. The chairman acted as rapporteur. Each reviewer was asked whether the comments they submitted (Appendix 5) were accurately addressed in the revised manuscript. All reviewers agreed that their concerns had been met, or else an ommission to do so was adequately explained by Mr. Amiro. Reviewers were specifically asked whether they agreed with the conclusions of the report. A consensus was reached on the wording of the conclusions and those are presented

in the final manuscript. No minority reports were aired. The review panel requested that a number of additional changes be made to the document, including a Table specifically listing the rivers that have endangered populations of inner Bay of Fundy salmon along with a summary of the data available for those rivers. In particular, sections of the report referring to the possible causes of the decline were carefully reviewed to ensure that all statements were well supported.

The final manuscript was referenced against the changes asked for by the working group, and signed off for COSEWIC submission by the chairman on September 29, 1999.

MINUTES OF THE SEPTEMBER 27, 1999 CONFERENCE CALL

Working Paper: Amiro, Peter G. 1999. Population status of inner Bay of Fundy Atlantic salmon (*Salmo salar*). Draft Manuscript, DFO, Maritimes Region Working Paper 99/49.

Referees: Dr. R. Bradford, Dr. R. Doyle, Dr. E. Kenchington, Dr. J. Ritter, Dr. F. Whoriskey, Dr. B. Riddell (separate call)

Regrets: Dr. A. Boer, Dr. R. Saunders

Rapporteur: Dr. E. Kenchington

The following represents the major points raised by the review panel according to Sections of the COSEWIC document:

Description

Whoriskey: What rivers are we specifically requesting listing for?

Consensus Action: Inclusion of a Table listing the rivers under discussion with a cross-reference to available data sources for those rivers.

Population

Doyle: Population viability analyses (PVA) are useful in developing a proper strategy of what to do next in order to manage breeding programs and to set guidelines for re-introduction of hatchery reared fish.

Amiro: A PVA for these salmon produced a high level of error. Given that there is controversy in the literature over the interpretation of PVA, and that the analysis is not robust in this situation, a PVA was not included in the COSEWIC status report. It was felt that the high level of uncertainty might detract from the other data sources which themselves clearly indicate population trend.

Consensus Action: It was agreed that a PVA would detract from the status report and should not be included however it was recommended that research in the area of establishing minimum viable population size is necessary in order to address the point raised by Dr. Doyle.

Habitat

Issue: Freshwater Habitat

Doyle: Soften the statement closing off freshwater habitat as a cause of the decline.

Bradford: You can't decouple the freshwater habitat from the marine as both are linked.
Ritter: The timescale of change may not be applicable to the loss. While no doubt development and other changes have influenced habitat over the last century, there have been no substantial changes to fish passage over the past decade, the period during which the population has declined and that is under discussion here.
Consensus Action: A statement in the Recommendation section was added to recommend ongoing monitoring of freshwater populations.

Bradford: Brown trout cannot be used as an indicater of freshwater habitat availability and suitability as it is a different species with different physiological and behavioural responses.
Amiro: Disagreement: Brown trout have very similar freshwater habitat responses to Atlantic salmon.
Consensus Action: No change.

Issue: Marine Habitat
The review panel was asked to review this section with regard to references to the impact of aquaculture on the salmon stocks. There was consensus opinion that the standing statements were appropriate and referenced accordingly. No action was requested.

Limiting Factors
Changes were asked for in this section to reflect the discussion of freshwater habitat noted above.

Evaluation
Whoriskey: There is evidence for a lower population of seals in the Bay of Fundy which contradicts unreferenced statements in this section.
Consensus Action: Statements referring to an increase in the seal population should be removed.

Status Recommendation
A consensus on the evaluation and status recommendation was reached by the review panel:

1) the uniqueness of the population (phenotypic and genetic),
2) the small population size (less than 500 adults),
3) steep decline in the population (≈90% in ten years),
4) the severe under-distribution (79% of sampling locations in the Stewiacke River were void of age-0' parr in 1999),
5) survival from smolt to first spawning is extremely low
6) a decreased longevity
7) the absence of any river population greater than the conservation requirement from which re-population of extirpated inner Bay of Fundy rivers could occur naturally,

a status of ENDANGERED was recommended.

REFERENCES

DFO Science. 1999. Atlantic Salmon Maritime Provinces Overview for 1998. Stock Status Report D3-14 (1999).

Marshall, T.L., G.J. Chaput, P.G. Amiro, D.K. Cairns, R.A. Jones, S.F. O'Neil, and J.A. Ritter. 1999. Assessments of Atlantic salmon stocks of the Maritimes Region, 1998. DFO Can. Stock Assess. Sec. Res. Doc. 99/25.

Appendix 1. List of Participants

Review Panel

Panel Members	Affiliation/Address	Telephone	Fax	E-mail
Boer, Dr. Arnold	NB Dept. Natural Resources and Energy/Fredericton, NB	506-453-2433	506-457-4881	aboer@gov.nb.ca
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Doyle, Dr. Roger	Genetic Computation, Ltd./Halifax, NS	902-420-0309	902-429-0074	rdoyle@is.dal.ca
Kenchington, Dr. Ellen	DFO, Science/ Maritimes Region	902-426-2030	902-426-1862	kenchingtone@mar.dfo-mpo.gc.ca
Riddell, Dr. Brian	DFO, Science/Pacific Region	250-756-7145	250-756-7053	riddellb@pac.dfo-mpo.gc.ca
Ritter, Dr. John	DFO, Science/Maritimes Region	902-426-3136	902-4266814	ritterja@mar.dfo-mpo.gc.ca
Saunders, Dr. Richard	DFO, Science/St. Andrew's, NB	506-529-3118	506-529-5862	saunpd@nbnet.nb.ca
Whoriskey, Dr. Fred	Atlantic Salmon Federation/ St. Andrew's, NB	506-529-1039	506-529-4985	asfres@nbnet.nb.ca

Other Reviewers

Reviewer	Affiliation/Address	Telephone	Fax	E-mail
Lacroix, Dr. Gilles	DFO, Oceans/St. Andrew's, NB	506-529-3348	506-529-5862	lacroixg@mar.dfo-mpo.gc.ca
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Powles, Dr. Howard	DFO, Science/Ottawa	613-990-0279	613-954-0807	powlesh@dfo-mpo.gc.ca
Price, Iola	DFO, Science/Ottawa	613-990-0275	613-954-0807	pricei@dfo-mpo.gc.ca

Appendix 2. Invitation Letter

Maritimes Region
Science Branch
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2

August 24, 1999

Distribution

Subject: DFO peer review of "Population status of inner Bay of Fundy Atlantic salmon (*Salmo salar*)"

A manuscript has been prepared by Mr. Peter Amiro, Science Branch, Bedford Institute of Oceanography, Department of Fisheries and Oceans, for submission through the Department to COSEWIC (Committee on the Status of Endangered Wildlife in Canada). The ADM Science has asked that such manuscripts be reviewed prior to submission to ensure that they meet the COSEWIC quidelines and that the scientific evidence is well documented and accurately portrayed.

The purposes of this peer review are: to determine whether the traits defining the population are distinct enough to warrant submission and whether the status "Endangered" is appropriate based upon the information provided; to identify important data that may have been neglected; to identify any weaknesses in the conclusions; to suggest areas where information is not appropriate or where additional information should be included; to help improve the clarity of the manuscript; and to ensure that the manuscript meets the COSEWIC guidelines for submission. The report should not include conjecture about what caused the decline of the resource, but should state what is known scientifically about this.

A copy of the Amiro manuscript will be sent to you electronically and information on COSEWIC can be obtained at http://www.cosewic.gc.ca/COSEWIC/Default.cfm although the link to the Guidelines for Status Report Preparation is not functional. A copy of those guidelines will be sent to you by fax, and hard copies of both documents will be sent to you by regular post.

All reviewers are asked to send their comments to Ellen Kenchington electronically or by post by September 10, 1999. Should there be disagreement amongst the reviewers on key issues, a conference call will be organized to permit discussion of those issues. The minutes of this call and/or the review submissions will be published as a proceedings, documenting both consensus and minority opinions.

We greatly appreciate your contribution to this valuable exercise.

E. Kenchington
Chairman of the Inner Bay of Fundy Salmon Working Group under the Regional Advisory Processs

c.c. : J.S. Loch
R. O'Boyle

Distribution:

Scientific referees

Dr. Arnold Boer
Dr. Rod Bradford (DFO-BIO)
Dr. Roger Doyle
Dr. Gilles Lacroix (DFO-STABS)
Dr. Brian Riddell (DFO-Naniamo)
Dr. Dick Saunders
Dr. Fred Whoriskey

<<Attached>>

COSEWIC Guidelines for Authors of Status Reports April 1997
COSEWIC Summary of COSEWIC Risk Categories and Criteria with suggested threshold values
Draft Manuscript, Population status of inner Bay of Fundy Atlantic salmon (*Salmo salar*). DFO Maritimes Region Working Paper 99/49.

Appendix 3. Meeting Remits

- Are the traits defining the Inner Bay of Fundy salmon population distinct enough to warrant submission?
- Is the recommended status "Endangered" appropriate based upon the information provided?
- Has important data been neglected in the proposal?
- Are there any weaknesses in the conclusions of the proposal?
- Does the report include conjecture about what caused the decline of the resource?
- Can areas be suggested where information is not appropriate or where additional information should be included?
- Can the clarity of the manuscript be improved?
- Does the manuscript meet the COSEWIC guidelines for submission?

Appendix 4. List of Documents Tabled

Amiro, Peter G. 1999. Population status of inner Bay of Fundy Atlantic salmon (*Salmo salar*). Draft Manuscript, DFO, Maritimes Region Working Paper 99/49.

COSEWIC Guidelines for Authors of Status Reports April 1997.

COSEWIC Summary of COSEWIC Risk Categories and Criteria with suggested threshold values.

Appendix 5. Reviewers written comments.

Review Panel

Dr. Arnold Boer, New Brunswick Department of Natural Resources and Energy, Fredericton, N.B. (email attachment):

Scientific evidence is well documented and accurately portrayed

- The author has done a good job capturing and describing the known status of the inner Bay of Fundy Atlantic salmon stocks.
- In the Section "Trends in Abundance", the estimates of historical adult population size have been calculated. The methodology is not questioned but rather the answer from the estimates indicate that as high as 43,000 salmon could have been produced. Are there sufficient freshwater habitats to rear the number of smolts for this adult population size and what would the marine survival rate have been?
- What were the reasons (known or suspected) of the low returns of adult Atlantic salmon in the late '50s and early 60's?
- Page 6 – Proper address is Tom Pettigrew, Regional Biologist, Department of Natural Resources and Energy, P.O. Box 150, Hampton, NB, E0G 1Z0. Pettigrew should also receive mention in the Acknowledgements.
- Some explanation of the following statements is required (General Biology page 11).
  "Compensation for highly variable marine survival can be seen in their higher annual survival to and between multiple spawning. Based on an index, recruit eggs equaled or exceeded spawner eggs only once in ten years (1975 to 1987) on Stewiacke River (Amiro MS 1987)."
- Reference to 30m tides should be corrected in Marine Section page 11.

Traits defining the population are distinct enough to warrant submission

- In the Section "Population Identity", the use of the term recruitment correlation requires clarification. If the author is referring to the historical high proportion of repeat spawners then this should be clearly stated. If it references the percentage of conservation requirement that is achieved annually or the average spawner to smolt production then similar explanation should be provided early within this section.
- The author has done a good job convincing the reader that the stocks are genetically distinct. However, some uncertainty exists that this genetic variance may have resulted from the infusion of intentional introduced foreign stocks. Although the statement is made that *"The genetic information suggests that the frequency of introduced genes in the present population is low"*, the evidence for this could be questioned.
- Overall the author has provided complete and accurate information to demonstrate that the population is a unique stock and that immediate and significant efforts are required to prevent extinction.

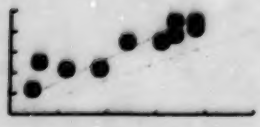
Is endangered the proper status?

- I think it is inappropriate for the author to suggest a specific status such as Endangered. COSEWIC is trying to be more objective and is currently changing it's assessment process to follow the IUCN model. The recommended status will be determined by the Species Specialist Group for marine fish based on a review of the information in the status report and the application of objective criteria. COSEWIC will review information and make a finai recommendation on the risk category.

Identify important data that has been neglected

- Additional scientific data has been recently collected on the Gulf of Maine and there may be some relationship with ecosystem change that may be occurring in the Gulf and the decline of Inner Bay of Fundy Atlantic salmon stocks. Some attention should be paid to this possible linkage.
- The statement *"The iBoF salmon population does not appear large enough to withstand the time required to collect the information necessary to resolve the collapse of the stock"* is fair. However, it should be suggested that some of this information could be collected simultaneously with implemented management options as part of an adaptive management approach.

Dr. Roger Doyle, Genetic Computation, Ltd, Halifax, N.S. (email attachment):
Document 1:



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Dr. E. Kenchington
Maritimes Region Science Branch
Fisheries and Oceans Canada
Bedford Institute of Oceanography
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8 September, 1999

Concerning *Population status of inner Bay of Fundy Atlantic salmon (Salmo salar)*, by Peter G. Amiro, Department of Fisheries and Oceans

1. Are the traits defining the population distinct enough to warrant submission to COSEWIC for classification as endangered?

The guidelines provided by the Committee on the Status of Endangered Wildlife in Canada (COSEWIC), following the U.S. Endangered Species Act (ESA, as amended), include distinct population units as well as true species in the definition of "species". In the U.S., a species is considered worthy of protection under the ESA if it is also an Evolutionarily Significant Unit (ESU) as the term is used by the Fish and Wildlife Service and the National Marine Fisheries Service. The genetic and other evidence discussed by Mr. Amiro would very probably show that iBoF salmon are a distinct ESU if the evidence were comprehensively analysed for that purpose.

An ESU is defined as a population that (1) is substantially reproductively isolated from nonspecific populations and (2) represents an important component of the *evolutionary legacy* of the species. It should be noted that some National Marine Fisheries Service (NMFS) rulings have used a significantly different wording of criterion (2), namely that the ESU should contribute substantially to the *ecological or genetic diversity* of the biological species. This ambiguity in interpretation of criterion (2) may be important in considering what practical steps should be taken to protect the iBoF salmon. (Section 8.)

Genetic distinctness

Moritz (1994) suggested that the genetic criteria for evolutionarily significant units should be that they show phylogeographic differentiation for mtDNA variants *and* significant divergence of allele frequencies at nuclear loci. Mr. Amiro cites evidence that both these criteria are likely to be met by the iBoF salmon. However, his manuscript does not contain anything like the comprehensive genetic analysis which was done in the U.S.A. for Pacific salmon populations which have been assigned endangered-species status [e.g. NOAA-NMFS-NWFSC TM-32: *Chum Status Review*; NOAA-NMFS-NWFSC TM-35: *Chinook Status Review*]. For this reason I am assuming that the manuscript is the equivalent to a "petition" for consideration by COSEWIC, and that COSEWIC or perhaps DFO itself would be responsible for conducting additional analyses if these are thought to be required.

The NMFS policy states that in general, "ESUs should correspond to more comprehensive units unless there is clear evidence that evolutionarily important differences exist between smaller population segments." On Pg5, Pa5 of the manuscript Mr. Amiro concludes that "This data is strongly supportive of iBoF stocks in general constituting a distinct intraspecific evolutionary grouping." Given the preliminary nature of the iBoF genetic analyses a more conservative conclusion might be something like, "These data strongly support the idea that at least one of the iBoF stocks, which together constitute a single conservation /management unit, also represents an evolutionarily significant unit (ESU) as defined by Waples (1991), Moritz (1994) and others."

Genetic diversity or evolutionary legacy (?)

The contribution of the iBoF to *genetic diversity* of the species as a whole can be calculated if this is thought to be necessary. The published data mentioned by Mr. Amiro would seem to be sufficient for estimating the contribution of a population such as the Stewiacke to the total species genetic diversity. (It is a function of the within-population diversity of a single subpopulation and the weighted sum of Nei's minimum genetic distance to all subpopulations (Finkeldy and Murillo 1999)). Whether the contribution would, if calculated, turn out to be "substantial" would then become a subject for debate. I believe Mr. Amiro makes a good case for supposing that it would be substantial. The iBoF is probably an ESU when the genetic diversity criterion is used.

Apparently in the U.S. "evolutionary legacy" of a species means the genetic variability that is a product of past evolutionary events and that represents the reservoir upon which future evolutionary potential depends. Conservation of this reservoir should help to ensure that the course of evolution will not be unduly constrained in the future. The significance of this reading of the ESU definition is that the *evolutionary legacy* of the iBoF as it now exists may not be entirely favorable to its survival and continued evolution. Preserving the legacy may in fact constrain survival, which is hardly what is desired. See section 8.

In summary, I believe that Mr. Amiro has presented a good preliminary case (a petition) for calling the iBoF distinct species (ESU) in need of protection according to the intention of COSEWIC. The need for additional analyses of the existing genetic data would increase more or

less in proportion to the likelihood that a ruling would be challenged by commercial or other interests.

2. Is the status "Endangered" appropriate based upon the information provided?

A species may be classified "endangered" when it is in danger of extinction within the foreseeable future throughout all or a significant portion of its range. A "threatened" classification is provided to those animals likely to become endangered within the foreseeable future throughout all or a significant portion of their ranges.

Variation in production and/or survival is, along with long-term trends and current abundance, a primary determinant of extinction risk (Caughley 1994). Conservation biologists have argued that the process of extinction is best viewed as stochastic, and that endangerment should therefore be defined probabilistically, that is, in terms of the probability of persistence over time.

Salmon abundance tends to be highly variable, with interannual fluctuations in the range of 40-70% (Bisson et al. 1997). Bisson points out that because salmon have evolved and are adapted to variable systems, variation in itself is not an indicator of risk to healthy populations. This point is also made by Amiro and is one of his arguments for considering the iBoF a valuable population.

To my eye the evidence presented by Mr. Amiro for a recent, catastrophic and unusual decline in the iBoF is somewhat less convincing than a strong endorsement of endangered status would require. For example, while Figure 6 (Stewiacke river sites devoid of salmon fry) does support the inference of a regular downward trend, Figure 7 (parr densities in the Big Salmon River) shows that 1996/1997 had the second-highest peaks since records began in 1968. Figure 3 doesn't show much of a trend at all. Furthermore there are the century-old papers referred to by Mr. Amiro which note the irregular abundance and possible extinction of the Bay of Fundy salmon. While it is probably justified, the present level of concern about immanent extinction is not unprecedented.

Historical fluctuations in abundance of the iBoF make it hard to distinguish a new signal, a recent, urgent -- and anthropogenic -- problem, from background noise. Is there conclusive proof of an unusual trend or imminent collapse? There is no quantitative analysis in Mr. Amiro's paper which proves that similar rises do not happen again and again. A critic of the proposal to list the iBoF as endangered might argue that Canadian regulators should not be concerned with the status of populations that are chronically on the verge of extinction in their natural state.

Population size estimates are crucial, not merely for the determination of endangered status but also for subsequent decisions as to what sort of supportive breeding programme (if any) should be undertaken. During the past decade international scientific discussion of this issue has centred on the concepts of Population Viability Analysis (PVA) and determination of the size of Minimum Viable Populations (MVP). See for example Mann and Plummer (1999) for a recent critique of PVA and MVP. For small populations that are stable or increasing, population size is

an indicator of whether the population can sustain itself into the future in the face of environmental fluctuations and small-population stochasticity; for a declining population, the present abundance is an indicator of the expected time until the population reaches critically low numbers.

It appears that Mr. Amiro's procedures for calculating sustainable population sizes are highly specific to salmon and have been worked out within the DOF to deal with specific local situations (I may be wrong here.). Stochasticity has not been taken into account in the calculation of probable survival time. If criticism is anticipated, the procedures used for decision-making should be consistent with recent scientific thinking. In Section 6 I have noted that clarification of the manuscript is needed in this crucial area.

3. Have important data been neglected?

(See comments on habitat data, Section 5)

4. Are there weaknesses in the conclusions?

The author has made an excellent petition for listing the iBoF salmon as endangered, but I suspect that as it now stands the petition would not survive a strong technical attack, e.g. by aquaculture interests or others who might be opposed to hypothetical regulatory measures. Mr. Amiro shows a deep understanding of Atlantic salmon biology, fisheries and ecology, but other technical fields are also involved in the determination. Perhaps a truly convincing recommendation would have to come from a committee of experts rather than one author, as has indeed been the approach followed in the U.S. Having said that, however, let me say that I think Mr. Amiro has done an excellent job and that his conclusions would be strengthened, not overturned, by more detailed analysis.

5. Are there areas where information is not appropriate or more information should be included?

The published and unpublished data available on iBoF salmon should be sufficient for population viability analysis and estimation of the minimum viable population. I emphasise that such a PVA would not supplant the DOF calculations and conservation requirements which are outlined on p. 8 of Mr. Amiro's manuscript (assuming that more details are provided). The objective should be to relate quantitatively his estimate of the conservation requirements to historical fluctuations in iBoF salmon, using the methods of demographic and genetic risk analysis. A second objective would be to see whether his conservation requirements are conservative, excessive or bang-on according to calculations based on other more widely-used approaches.

The COSEWIC *Guidelines for Authors of Status Reports* (page 2) states that reports should emphasise habitat needs and problems. This is the modern trend in wildlife conservation.

As far as fresh water habitat is concerned Mr. Amiro's manuscript concludes that "there appears to be little change in the capacity of the iBoF rivers to produce juvenile salmon". However, there is no direct, quantitative information provided on this point. There are no estimates, for example, of recent intensification of farming or logging activities, agricultural use of chemicals in the watershed, acidification and other changes in water quality, fragmentation and loss of available spawning and rearing habitat, alteration of streamflows and streambank and channel morphology, unusual migration delays, alteration of ambient stream water temperatures, sedimentation, loss of spawning gravel, pool habitat and large woody debris, removal of riparian vegetation, and decline of habitat complexity.

The argument Mr. Amiro uses to support his conclusion about the adequacy of freshwater habitat is indirect and uses ratios of repeat spawners, recruits and eggs in a way I don't understand. As mentioned in the next section the manuscript needs to be expanded in this area to become convincing or at least self-sufficient. The COSEWIC instructions pose a lot of questions of the sort "fraction of habitat degraded?". COSEWIC's interest in habitat problems may not be satisfied by the manuscript.

6. Suggestions for improving the clarity of the manuscript

Pg 8, Pa 1. The author's determination of the conservation requirements (minimum sustainable size of the breeding population) is not explained well enough on Pg8, Pa1 and Pg12, Pa4. I honestly can't follow it at all. Probably to save space, reference is made to other government manuscripts which are not conveniently accessible. I urge Mr. Amiro to add as many paragraphs as it takes to explain how his iBoF salmon conservation requirement was determined and to present the relevant data.

pg5, pa3. "Quantitative genetic" ... Should be "population genetic" here and elsewhere in the manuscript. I can see that the author is using "quantitative" to mean that genetic data have been obtained and analysed, but "quantitative genetics" is a defined subspecialty of genetics and his use of the term here is definitely misleading. It implies that someone has comparative genetic data on quantitative traits such as maturation and growth. I wish that were true. The study of gene frequency data is properly called "population genetics".

pg5, pa5 "Mitochondria DNA analysis" should read "Mitochondrial DNA analysis".

Pg11, Pa4. The index should be specified.

7. Does the manuscript meet the COSEWIC guidelines for submission?

The manuscript is well written and organized but does not closely follow the COSEWIC recommended format. It would be an easy matter to re-arrange the material. This might be worth

doing to assist the committee in its work but in my opinion is not necessary for any other reason. COSEWIC has a summary checklist form that should be filled in.

8. Comment on the gene bank

Mr. Amiro indicates that as an emergency measure the DOF. has already undertaken a parr-smolt rearing and perhaps breeding programme, and recommends that this be continued and expanded. *I concur with this recommendation and think that starting this programme in a timely way was a very good move.*

I do have some concerns about the gene banking programme however. The iBoF populations are now and often have been small, fluctuating and subject to bottlenecks in the genetic sense. As part of his evidence for this Mr. Amiro says that the iBoF salmon show special life history adaptations which have evolved in response to the demographic fluctuations in marine survival (Pg12, Pa1). If so, these adaptations would certainly enhance the evolutionary value of the population unit.

Unfortunately, the same population bottlenecks can have other effects including acceleration of random genetic drift and loss of heterozygosity. The practical implication for gene banking is that recent bottlenecks may already have given a population such as the Stewiacke an unfortunate genetic legacy of inbreeding depression.

It has been found through Leslie matrix simulation that slowly-growing populations are likely to be very sensitive even to very low levels of inbreeding depression (Mills and Smouse 1994). Recent work in my laboratory (Gadagkar 1998) suggests that inbreeding depression (in tilapia) is especially hard on the survival and maturation of fish *when they are competing with other, less inbred fish*. Gadagkar found that inbreeding effects in competitive situations are much greater than have been previously reported for inbred fish reared alone, e.g. in the classic experiments of Kincaid on rainbow trout (Kincaid 1976). A similar phenomenon has been shown in Atlantic salmon in a smaller experiment by Herbinger (Doyle et al. 1995).

Theoretical opinion about the effect of inbreeding on the viability of small populations is mixed, ranging from an early statement that the effect of inbreeding depression is small relative to other random catastrophes (Lande 1993), to more recent and completely opposite calculations (Lynch, Conery, and Burger 1995; Vucetich and Waite 1999). The problem is that when populations become small they also become inbred, which increases their sensitivity to environmental stress and further reduces their size, which accelerates the accumulation of inbreeding... and so on to extinction. The iBoF salmon may already be caught what has been termed an "extinction vortex".

The practical implication of a possible historical legacy of inbreeding depression in the iBoF arises from the fact that that *no amount of careful inbreeding avoidance within a single, captive population can ever remove it*. Additional measures must be taken. This possibility should be addressed quickly, before supportive breeding is begun in the Stewiacke rearing facility.

Historical inbreeding has obvious implications for the captive breeding strategy that must be followed to resurrect a genetically healthy, competitive iBoF population which still retains its evolutionary value.

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Document 2:

Dr. E. Kenchington
Maritimes Region Science Branch
Fisheries and Oceans Canada
Bedford Institute of Oceanography
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Dartmouth, Nova Scotia B2Y 4A2

22 September, 1999

Concerning notes on the inner Bay of Fundy Atlantic salmon

Dear Ellen:

While working on the Amiro manuscript I jotted down a few thoughts on the iBoF salmon situation that weren't directly applicable to the review itself. For the sake of completeness I' thought I'd better send you these notes prior to our conference call.

How to strengthen the case for a recent and unusual iBoF decline. This can be done quickly by re-analysing existing genetic data for the specific purpose of finding effective population sizes and recent "bottlenecks". None of the papers I've seen have done this type of analysis. An elementary statistical analysis of the existing population data should also be done, for the same specific objectives, and this analysis should include power analyses and estimates of the cost of Type II error (the cost of mistakenly saying that the iBoF is not in decline when it really is).

How to strengthen the case that the iBoF is worth saving. There is a need for a quick but comprehensive analysis of the existing genetic, morphological, behavioral and biogeographic data to estimate the evolutionary significance of the iBoF. No combined analysis has yet been done.

How to strengthen the case that intervention will be required to save the iBoF. The quickest way to do this is subject the available data to a formal population viability analysis (PVA) using the VORTEX or RAMASS programs or something similar. Such analyses give estimates of the expected time-to-extinction of populations. They also provide "sensitivity analyses" – essentially, models within which various protection/intervention scenarios can by played out on a "what if?" basis. Although the model output has to be taken with a lot of salt it would give some notion of the effects of increasing spawning habitat area or improving survival at various life history stages.

When to identify the causes of the iBoF problem(s). Realistically, the causes will never be known with certainty even though some causes may be more obvious than others. The formal PVA plus the arguments in Mr. Amiro's paper will certainly provide some guidance for research proposals. My view on when to do this research may be a bit odd. I think we

should simply declare that the search for causes will go on indefinitely! Remedial actions can be assessed continuously in the light of the best current data. The search for the causes of the iBoF decline would not be a preliminary to taking action; it would itself become part of the action. This is very different from the approach currently taken in the USA.

What should be done? Proposed remedial actions could be ranked in four ways, (a) on the basis of our certainty that they will work, (b) their benefit to the ESU if they do work, (c) their direct implementation cost, (d) their social costs and benefits on various time scales.

Supportive breeding of the iBoF salmon will surely work if it is properly done, and it has a low short term social cost and high long-term benefit, but it is costly to implement. It is an excellent thing to do on the basis of our current understanding. The benefit to the iBoF (criterion b) has yet to be estimated numerically but I think that this could also be done on a monetary basis, like the other ranking criteria except (a). Criterion (a) – our certainty that the action will be effective – is a dimensionless number and so it can be combined with the other four criteria to make a decision model.

It is this model (actually a sort of assessment) which would be updated continuously as new information comes in. This, too, is very different from the American approach.

Sincerely,

Roger

Roger W. Doyle

Home phone: 902-420-0017
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Dr. Brian Riddell, Science Branch, Department of Fisheries and Oceans, Nanaimo, B.C. (email attachment):

Review: P.G. Amiro. Population status of inner Bay of Fundy Atlantic Salmon

This draft manuscript has been prepared for submission to COSEWIC as a status report on this stock group of Atlantic salmon. As such the document should provide the evidence/data upon which COSEWIC may judge the status of this resource. While the manuscript has been prepared according to the "approved format" (at least according to the COSEWIC guidelines to authors and my limited experience in these documents), my only serious concern for the manuscript is the summary level at which the detail is presented. The author cites many other DFO documents but many of these may not be readily available to the COSEWIC members. My recommendation would be to provide more data and analyses within this manuscript. I am particularly struck by the lack of analyses given that the eventual conclusion is a recommendation of "Endangered". The only deviation from the guidelines may be the inclusion of the Population Identity and Genetic Information sections within the section on Population Size and Trends. Such information on the uniqueness of this stock group seems more suited to the "Special Significance" section according to the guidelines. I agree with the author, however, that this stock group is unique and that it merits submission to COSEWIC.

Concerning the data presentation, you seem to suffer from the same fragmentation of data (bits and pieces from various times, streams, gears, etc.) that we have on the Pacific. Given the audience for this report, it maybe advisable to acknowledge this concern and how you have dealt with this. For example, relying on the weight of evidence from several populations or relying on a couple of populations.

An editorial matter is that the Introduction (last sentence) states that the report "evaluates the prospect for persistence of wild iBoF Atlantic salmon". There is no single section that summarizes the basis for the recommendation of "Endangered" nor is there any evaluation of persistence. I would recommend such a summary is necessary but the evaluation would likely be a new analysis. The limited data presented does seem to support the recommendation but my sense from the report is that this recommendation is inferred from the data as opposed to assessed in a more technical manner (for example, life cycle models or risk assessments).

I have a substantial problem that the report equates a "conservation limit" with the number of spawners that maximizes production. This is an on-going debate in the Pacific but is also one larger than this report. The author's use of this definition is, however, consistent with the CAFSAC documents cited.

There were a few points of clarification for the Trends in Abundance section:

1) Figure 2 shows a major change in catch between 1915-1920. The stock (based on catch) never seemed to recovery from this change … was this first major impact on the stock or does the catch reflect new regulation as opposed to abundance?

2) Is Figure 3 a stacked bar graph so that the top line is the cumulative total? If so, I do not agree that catches were "often between 2,500 and 3,000 fish" (page 6). The usual values were between 1,500 and 2,500; which could be substantially less than the 11,000 escapement stated on page 6.
3) Is the assumed 0.25 recreational harvest rate applicable to these rivers ... was the effort comparable; is there any basis for assuming this value?
4) The Stock Status section on page 7 refers to ten rivers monitored in the Bay of Fundy ... where is this data and how often is "conservation" not met? Is there synchrony between streams?
5) For clarity, I would suggest all "Trend" type data (adult and juvenile surveys) be aggregated in the Trend section and all the "Status" info collated in that section (for example, the 1998 Big Salmon River comments on page 6).

On page 8, I see no value to the second paragraph. It would be a re-iteration of information in the Trends section. Further, in the next paragraph, the comments on "repeat-spawning salmon component" may be very misleading. It is true that repeat spawners may increase the observed numbers of juveniles, but the longer-term viability of the stock may be seriously compromised by this life history feature. The genetically effective population size will be rapidly decreased as the relative numbers of repeat spawners increases, simply by increasing the genetic relatedness of the progeny.

Are there any long-term databases on the marine survival of Atlantic salmon from that Region? Can you actually separate variation in marine survival from changes in exploitation over time? Your data on these topics seems very limited, but merits better description of the data and limitations. After all, it does become the central conclusion of the paper (page 12). I presume there is a technical basis to the statements concerning the negative impacts of commercial fish farming (the citation presented is an MS) and the general acceptance of this??

Recommendation section: I would urge a strong recommendation to pedigree the stock immediately and to take this program very seriously (see Waples and Chi Do. 1994. CJFAS 51(Suppl. 1): 310-329). Without very careful management this "live gene bank" may simply become an expensive means to inbreed the stock that is already at risk. In my opinion, a live gene bank without a pedigree program is not a responsible conservation action.

Evaluation: This section should provide a succinct summary of the reason for the status recommendation. For example, the second sentence may not be true, but is irrelevant. Any population is at risk if the rate of population growth is below its rate of replacement ... especially over the time period documented here. Finally, it seems more appropriate to state that the stock is at increasing risk of extinction, as opposed to that "the stock is likely to become extinct". There has not been any quantification of the rate of decline in population abundance, so how has the recommendation of "Endangered" been arrived at?

Dr. Richard Saunders, DFO, St. Andrew's Biological Station, St. Andrew's, NB (reproduction of letter sent by post):

Population status of inner Bay of Fundy Atlantic salmon (Salmo salar)

Peter, I think that you have done a good job making the case for getting the iBoF salmon designated as endangered. I hope that you are successful. I have a few suggestions that are meant to improve your case or make things clearer:

It would have been easier to make comments on the manuscript had you used double or triple space!

The copy of guidelines for authors for status reports called for an executive summary rather than an abstract.

I think we differ on this point but I suggest you use the definition of stock given by Ricker and say assemblage or collection of stocks in the inner Bay of Fundy. You could get around this by referring to them as iBoF salmon rather than iBoF stocks of salmon, or assemblage of stocks. I have made comments on the manuscript wherever this was indicated.

There should be a caption and title for Figure 1. Also, why not number the rivers in sequence, as suggested on the manuscript?

There are salmon in Tasmania from the River Phillip since the 1960s. These are in aquaculture operations.

I couldn't see the data in Figure 2. Figure 6 was black too but I could see the bars.

Probably you have talked with Mike Dadswell about iBoF salmon. He told me about them in connection with his shad studies.

Caption of Figure 7 should say that there are no data for certain years.

Are you sure that oBoF salmon are not found in the Western Bay of Fundy in the fall? Possibly true but are you sure? You musn't say anything that will weaken your case.

Are you sure that seaward migration begins in the fall? I am surprised to hear that this occurs in such short rivers.

I suggest you say there is considerable evidence but not acceptance that aquaculture has something to do with survival of iBoF salmon.

That's all; good luck!
Dick

Dr. Fred Whoriskey, Vice President, Research and Environment, Atlantic Salmon Federation, Box 429, St. Andrews, NB., E0G 2X0 (email attachment):

Comments on the the Draft MS. « Population status of inner Bay of Fundy Atlantic salmon (*Salmo salar*) », by Peter G. Amiro.

Fred Whoriskey, Vice President, Research and Environment, Atlantic Salmon Federation, Box 429, St. Andrews, NB., E0G 2X0 tel 506 529 1039, Fax 506 529 4985, email asfres@nbnet.nb.ca

1) Dr. Amiro is the right person to have prepared this. He has extensive personal experience with the subject, and is a dogged, dedicated investigator with fist rate scientific credentials.

2) The information presented in the document leaves me convinced that the unique Atlantic salmon of the Bay of Fundy region are at risk of biological extinction.

3) I fear that the doument will be difficult for COSEWIC committee members, and the public at large, to understand. The principal problem is that the author has assumed that readers will be familiar with the Department of Fisheries and Oceans management and stock assessment regimes for Atlantic salmon. While DFO`s approach is highly respected and being adopted world wide, it is not intuitively obvious to an outsider.

I suggest that a section at the start of the document be included which outlines the stock assessment methodologies. It should include a description of the life cycle of the salmon, defining terms like parr, smolt, grilse, salmon , multisea winter fish, conservation limits (this is done late in the document, well after they are first being discussed), and repeat spawners. The justificication for the use of indicator rivers to monitor regional groups of salmon should be given, and descriptions provided for how adult counts were arrived at, the purpose of in-season reviews, the functions of Zonal Management committees, and the determination and importance (as an early warning system) of juvenile salmon abundances. All of these terms or titles are used in the text.

I had the following additional comments :

4) The « iBof» abbreviation needs to be defined in the abstract, where it is first used. Also in the abstract, indicate the purpose of the recommended supportive rearing to maturity of wild parr (e.g., broodstock versus experimentation).
5) P.3, paragraph 2, you mean bordering, not boarding. Also, I found the phrase «that contain salmon that are typical multi-sea-winter fish» confusing. I think that what is being driven at is the idea that the life histories of the adults in in these runs includes a large fraction of fish, typically females, which spend two or more years at sea before returning to spawn for the first time.
6) P.4 Include French name(Saumon atlantique) in the taxonomy section?

7) P.4, Distribution . To my knowledge, the Hudson Bay salmon are limited to a single relict population in the Nastapoka rier. Also, the eastermost distribution of the species is in Russia`s Pechora River.
8) P.4, Protection section. Protection is not provided by the Fisheries Act. It is mandated there, but provided by enforcement personnel. Also, I found the tag recoveries reported in the third paragraph of this section out of place. It is better concentrated in a single place, probably the marine habitat section.
9) P. 5, Population identity section. You need to identify here that the Stewiacke and Big salmon rivers are the indicator rivers for inner Bay of Fundy populations, and the reasons why they were selected. Also, I found the references to smolt tagging and recovery studies (3rd par,) confusing here, because the nature and objectives of these studies have not been given. I think all references to smolt tagging work are best kept in one section, probably under marine habitat.
10) P. 5, Genetic information. This is crucial for establishing the credibility of the biological uniqueness of inner bay populations. Much of the information is coming from unpublished sutdies, hence they can not be consulted if needed for additional details. I feel it is necessary to give many more details here (methods, probes used, procedures), so that the COSEWIC committee can convince itself of the validity of the assertions being made about the genetic lineages. In the Case of the Big salmon and Stewiacke rivers, their genetic similarity to each other and the rest of the inner Bay of Fundy river populations should be gone over. This is important if the Big Salmon and Stewiacke Rivers are going to serve as the source populations for recoveries of other rivers, if necessary.
11) Genetic information section, 2nd par. The Vespoor et al personal communication citation is inconsistent with the others given in the text. Elsehwere, addresses are given in footnotes for those providing personal communications. Also in this paragraph, I would strike « intraspecific» in the 2nd to last line, so that the text reads «a distinct evolutionary grouping».
12) P6, Fig 2. This was an unreadable black blob in my copy. Also, no address is given for the R. W. Dunfield personal communication in the Figure caption.
13) Trends in abundance section, p. 6, 3rd paragraph. Instead of a «0.25 exploitation rate», which readers unfamiliar with salmon may not understand, could the document read «at an assumed harvest of 25% of the salmon returning»?
14) Stock status section, p. 7. You have not yet defined what meeting conservation means. This needs to be done for this paragraph to be understandable. Also, you need to describe the way agreements are arrived at with First Nations for fishing, and the significance of not having one for the Bay of Fundy rivers. Is this an implied criticism? Or does it highlight the fact that the First Nations recognize and support drastic conservation measures for these populations?
15) P.8, first paragraph. This discussion of the setting of conservation limits needs to be moved to the start of the document.
16) P.8, 3rd paragraph. The discussion here of water quality issues seems out of place and probably would be best done in the freshwater habitat section. Also in this paragraph, the link between the rate of parr decline and a high repeat spawning component (which has not been defined to start with) is unclear. This needs to be beefed up.
17) P. 10, Freshwater habitat. I found this one of the weakest sections of the paper. No information is given on how habitat quality was evaluated, and on any existing programs to determine water quality and changes in other habitat features. The absence of information on

habitat quality, because there is no monitoring underway, should not be construed as there being no evidence for habitat loss.

18) Marine habitat section, p. 11. Much more information needs to be given on the tagging programs that were undertaken, and the recapture times and places of the fish that were recovered. Are the two fish returned from outside of the Bay of Fundy a signfiicant fraction of all the returns? If so, what leads you to conclude that the fish reside in the Bay of Fundy? I know this is not true, but some excellent and convincing work has been done and is not adequately described in this paper.

19) General Biology section. Confine information on marine movements to the marine habitat section. I do not undersand the statement about «Compensation for highly variable marine survival can be seen in their higher annual survival to and between multiple spawnings». Does this mean that the fish make a conscious decision to survive better? Also, I do not follow the sentence starting «Based on an index…». What index?

20) 3rd par, General biology section, recipient should be recipients. Also, I am not clear on why the reconditioned precocious parr migrating as smolts will poise the population for rapid recovery.

21) Special significance of the species section, par 2, p.12. This paragraph documents who is not interested in saving the inner Bay of Fundy salmon. What about all of us who are? I also think a major contributor to public confusion over the issue is that it is difficult to understand how the species can be in danger of extinction, when there are 6 million salmon swimming around in Bay of Fundy aquaculture cages. Finally, I am unaware of any group which is actively promoting the extinction of the Bay of Fundy salmon. Overall, I feel that this paragraph leaves a sense of a lack of support for iBof salmon which is false.

22) P. 13, 1st paragraph. I find the whole paragraph on outer bay stocks distracting and irrelevant to the focus of the paper. I would strike it.

23) Recommendations section. Need to establish the rationale for why these two stocks were chosen for gene banking.

Other Participants

Dave Meerburg, Science Branch, Department of Fisheries and Oceans, Ottawa (email attachment):

Comments on Amiro - "Population status of inner Bay of Fundy Atlantic salmon (*Salmo salar*) "

Overall, I found the paper to make a generally convincing case for the recommended status of ENDANGERED however it could benefit from some: reorganization, clarification in some areas and expansion in others to improve its readability for individuals unfamiliar with salmon biology.

I feel that information that documents the uniqueness of the salmon in this group of rivers should be brought close to the front of the paper; to demonstrate uniqueness may also require more reference to other stocks (with references) to show differences in biological characteristics, migration routes, tagging results, etc.

Also, I am not sure if it is necessary in a paper for COSEWIC to speculate on the causes of a decline in a population; the most important point to make is the magnitude of the decline and the likelihood of recovery.

Specific Comments:
(note that there is a mixture of comments following, some major points and many minor editorial)

Page 2

Abstract:

1st line –define inner Bay of Fundy as iBoF

9th line –unprecedented low in 1998; is this the last year of sampling or do we have some summer 1999 juvenile info?

10th line – statement that "smolt migrations were monitored up to 1997" says nothing – do you mean that we had counts up to 1997 but no evidence of any downstream migrating smolts now or that program was discontinued; what were results to that year – were smolt nos. declining, stable or what? –how many years of smolt info exists; can we correlate smolt counts and juvenile density measurements?

10th line- the sentence "survival to spawning recruitment has declined to less than that required for poulation stability for at least the past ten years" is confusing; is the word recruitment necessary?

12th line- can the word deferring be replaced by delaying?

16th line- the word supportive does not seem necessary

Page 3

Introduction:

2nd para, 1st line, remove the ; before Maine and last line mentions "unique life history traits" – what are they? need to contrast with previous sentence that mentions MSW and North Atlantic migration

3rd para – brackets around Gaspereaux R, Kings Co, NS and delete e.g. BSR and Stewiake in next line; don't they all spawn for the first time as grilse then spawn in consecutive years (i.e. not MSW) and they have high frequency of repeat spawning, up to 5 or 6 times, is this the result of low marine mortality or high post-spawning survival to mended kelt stage or what?

figure – also need a map showing SFAs mentioned under Protection section, FSDs mentioned in figure 2

4th para – insert word "of" after abundance in 1st sentence; statement that abundance has been in decline since turn of 20th century is not supported by figure 2, if commercial catches are indicative of stock size; decline seemed to happen in mid 1920s and it is is hard to say if 1950s were any different than recent years; what is the important point about 1989? if this is referencing Figure 3 on angling, then catches were very low also in 1980, 1987 and 1988. In 3rd line, change management to managers, in 4th line, a , after reports, and delete "all"; in 5th line rephrase to "assessments in rivers (from two to eight annually) …"

Page 4:

Taxonomy – indent on "and Scott 1966)"

Distribution – a reference for the South American colonizations; to have colonizations means that you have to have spawning adults, progeny that grow to maturity and return to spawn and persist; was the pink salmon transplant to Newfoundland a colonization? they don't appear to have persisted. We can't yet say if there has been colonization in BC (spawning and perhaps progeny occur but not yet grown to maturity); does your modifier "possibly" refer to BC also?

Historic – if North Atlantic Ocean does not include Bay of Fundy (page 3 para 3) then North Atlantic Ocean cannot include Baltic, Barents and Lake Ontario; change word including to "and into"

Protection – 2nd para 3rd line – change to "progresses" and in next line does performance mean abundance? 7th line – should be "was closed" In para 3 which starts with the word "internationally", reference is made to PAB drift net, commercial in Maritime Provinces (define – NB,NS and PEI?) and commercial in insular Nfld. Shouldn't these be moved to previous para talking about national regulations and add in changes in Labrador commercial fisheries on recent years. At end of 5th line, delete word "however" and in 7th line, replace "national …fisheries. The most" to "marine fisheries distant from the Bay of Fundy. One was a southerly tag recovery from Swampscot, Mass., USA." It would be helpful here to have a map/figure showing tag placement location and numbers of recoveries and their location contrasting iBoF with adjacent river such as Saint John, also to put into context the 2 recoveries outside. How many were recovered "locally" and at what age – postsmolt or after 1 SW. In last line, Port–Aux-Basques and could you conclude with a statement to the effect that" hence distant water fisheries are not thought to have been a factor in the decline of iBoF salmon?"

Page 5

Population identity – 1st para 2nd line insert " between salmon returning to inner Bay of Fundy rivers" after similarities; in the 2nd para, it would be useful to spell out in more detail the recruitment and life history patterns documented in the literature to save readers from a search.

Genetic information – do you have to provide an address when twice referencing Verspoor unpublished info, similar to pers comm Pettigrew on following page? In 7th line 2nd para typo iBoF, not oBoF

Page 6:

Trends in abundance

Can figure 2 be put in kgs for consistency? Why the discontinuity at 1940 with different FSD's being graphed? In the 1st para 2nd line reference is only made to 1940 yet graph goes to 1984? Although 60,000 lb in 1915 may have been maximum, it seems totally out of line with avg of previous years of about 20000 lb.

In 2nd para, what does low average weight mean, compared to what?; this should have come from biological characteristics info on previous page (presumably the mean weight is low because they spawn first as grilse and then consecutive years but should be clearly explained). Where does low estimate of exploitation of 0.2 come from? and what is low relative to? Remember that these numbers are being used to adjust 60,000 lbs into stock size. Could exploitation have been unusually high in 1915 to triple the average catch. Population was therefore perhaps 43,000 in that one year but more likely 15,000 in 40 years previous to that, using the assumed weight and exploitation level. It is not acceptable to pick the largest catch in weight and then use a low estimate of exploitation rate to estimate the stock size. There needs to be a reference to the 0.25 value used for recreational fisheries and the phrase restated to "If one assumes a 0.25 ...." and as well, clarify that the escapement estimate of 11000 to rivers was prior to angling harvests; spawning escapement similarly calculated would have been about 8250. At the end of the para, the word "fact" in the 3rd line should be changed to "estimate". What is the source of the estimate that Stewiake and Big salmon make up more than 50% of the population (is it from angling info in stock status section or habitat area or something else?), and if 50% is close, how do less than 50 in Big Salmon plus 2 in NS (including Stewiake) translate into 500 adult salmon; why not only about 100?

Page 7:

Stock status – 1st line, the words salmon and grilse with length defn's may confuse people ; why not just say averaged 1462 fish. At bottom of page, where it says there were no fishing agreements with First Nations for harvest of salmon does this mean that there was no First Nations fishing and explain difference of members of Native Council and First Nations

Page 8:

2nd para on angling catches is not convincing; 1983 value picked is highest in time series since 1970; 1980 saw extremely low angling catches, even lower than 1987 and 1988 which paper indicates were key years influencing management strategy (=closure); what accounted for rebounding of angling catches in 1981? Figure 5 label should note that y axis is a mean number of fish per unit; how come this is not paralleled in Figure 7 on Big Salmon 1+ fish, if stocks are in synchrony in iBoF? Also low recreational catches in 1980 in Big Salmon did not seem to result in low 1982 1+ parr; in fact they were the highest recorded between 1968 and 1998 (Figure 7b).

Page 10:

Stock status – figure 7 does not seem to imply any change in juvenile populations in Big Salmon River

Habitat – mention should be made of agricultural activities as well as forestry

Page 11:

General Biology- as mentioned previously, much of this section should be nearer the start of paper to demonstrate the uniqueness of iBoF salmon. What is meant by "higher annual survival to and between multiple spawning"? What are values for other stocks versus iBoF salmon? The following sentence concerning recruit eggs and spawner eggs needs to be clarified for readers not used to dealing with these terms.

The second para of this section is confusing when speaking of smolt migration timing. First it says that movement to salt water occurs in late May or June then in next sentence it says that iBoF smolts migrate later… (in July). Was previous sentence referring to most other salmon stocks or iBoF salmon?

In third para, it should be clarified if transferred stocks to Big Salmon were MSW stocks.

Page 12:

1st line – define what re-conditioned precocious parr are for readers.

2nd para, 6th line – what changes in harvesting regulations of marine mammals are relevant? what species?

at end of para, reference is made that " there is considerable acceptance of a negative interaction with fish farming" While there may be a negative correlation bewteen the two, it is going too far to say that there is a confirmed interaction.

Limiting Factors – it is not possible to compare wild smolt survivals in the Big Salmon river to hatchery reared smolt survivals in the Stewiake; What were return rates for other hatchery reared salmon smolts at the time of the 0.0 – 0.42% survivals. This section needs to be explained better; the first sentence of para 2 confuses the logic. I think what is being said is that the BSR can produce 25,000 smolts maximum (reference?) and requires at least 700 salmon spawners to do so. Survival therefore of the 25000 smolts must be at least 2.8% for the salmon to replace themselves in the next generation i.e. 25000 * .028 = 700. We have detected survivals of 0 to 0.42% of hatchery reared smolts and based on LaHave wild /hatchery smolt survival ratios, we might expect the wild survivals of Stewiake River fish to have been between 0 and 1.7%; where is the logic that says that if Big Salmon survivals need to be 2.8%, Stewiake River survivals at sea must be the same (isn't this an apples and oranges comparison?)

In 3rd para, it is not clear where statement arose that there has been little change in capacity of iBoF rivers to produce juvenile salmon.

Special significance of the species

in 5th sentence I am not clear what the Gulf of Maine Committee is and why salmon being a commercial species is relevant to this. Salmon have not been a commercial species in this area since 1984

Page 13:
define trans-boundary as meaning they migrate to Nfld-Labrador waters and Greenland

Recommendations – 2nd para in the 2nd sentence the words iBoF salmon stocks is used yet throughout the rest of the paper the iBoF is treated and considered almost as a singular stock. In 2nd last line, do you mean "expanded"?

Evaluation – in 3rd line, rephrase to something like " better suited to low and highly variable marine survival than most Atlantic salmon stocks due to their high survival to multiple spawning and low marine exploitation, they are nonetheless below a minimum survival threshold." In the 3rd para, there is mention of the concept of refuges for the first time, this idea should be developed more thoroughly earlier on.

Dr. Howard Powles, Science Branch, Department of Fisheries and Oceans, Ottawa (email attachment):

Comments - H. Powles August 29, 1999

Population status of inner Bay of Fundy Atlantic salmon (Salmo salar)

A lot of information is given to show that this population is at very low abundance, and the conclusion that this population is in serious trouble seems inescapable, but much of the information is not very well marshalled or organised and the information presented raises some questions. It could be made a lot stronger with some additional information, interpretation and reorganisation.

The evidence that this is a « unique population » is not very comprehensive or well documented. Under Population Identity (page 5) we read that this population is unique based on life history characteristics and recruitment correlation, but none of this information is shown in any detail. There are references elsewhere in the document to tagging and recruitment correlation information, but this is not provided or summarised. Recruitment correlation might show that they are similar but not necessarily unique -- unless their recruitment patterns are uniquely different from those in nearby stocks. And WHAT life history information ? growth ? or what ? I understood that there was tagging information that shows that these fish stay in the Bay -- this is not shown in detail although it is referred to in passing. More detail on the tagging and life history information, including some figures or tables, would strengthen the case for uniqueness considerably. On the other hand the genetic information is presented quite well and is convincing; following the same approach to summarise the tagging and life history information would be good. It is quite important that this document summarise all the « unique population » information since this is the first time a salmon population is being brought before COSEWIC.

The « Trends in Abundance » section paints a picture which is rather different than the line in the abstract : « an acute decline has taken place since 1989 ». Abundance actually declined dramatically in the early 1920's and has been quite low since then, if the landings are to be taken as an indicator (Fig. 2). The decline from 60,000 lbs (43,000 fish) in 1915 is referred to as an indicator of status, but in fact the 60,000 lbs was in one year only and possibly anomalous or erroneous, and the population was typically at a maximum of 20,000-30,000 lbs pre-1920's. The population has fluctuated considerably over the years, both in the « abundant » period pre 1920 and more recently -- is there anything we can learn from these considerable fluctuations ? Is this a population which was inherently more vulnerable that other Atlantic salmon populations in its natural state?

How does figure 4 relate to figure 2? the latter seems to show essentially zero catches since 1970, the former catches of above 1000 fish up til 1980 -- which should be about the equivalent in pounds.

The increase in 0 catches of fry (Fig 6) is more convincing but only occurs in one river. Parr density (Fig 5) also looks like a trend but the confidence limits are so wide that the decline is most likely not significantly different from zero. Fig 7 shows very low densities but no recent trend. The information that these rivers are below conservation requirements would probably be the most convincing that there is a serious problem, but no estimate of what the eggs/m2 actually might be in recent years is given.

From the information presented this looks like a situation of a very long-term reduction in abundance, of a population which even long ago was subject to large fluctuations. The recent decline (if there is indeed a recent decline) is superimposed on that long-term pattern and has brought the stock to a critically low state. Given that abundance really declined precipitously in the 1920's, the reasons for the decline should be re-examined -- couldn't it have been forestry or dams or some habitat factor back then ? It looks to me like a classic case of « cascading » effects such as is seen in some Pacific stocks -- habitat reduction in the early days bringing the population down a great whack, then gradual further decline because of overexploitation or other factors until the accumulation of threats (and perhaps a final new threat such as poor marine survival) puts the population « over the edge ». A rigorous description of the population trend is absolutely necessary if we are to try to understand what the threats are and what to do about them.

Practically no convincing information is given on current or historical threats -- no real evidence is given for the statement that freshwater habitat is not the problem (lack of prosecutions is not a credible index), and none for any marine sources of mortality either. It seems likely that the current problem is low marine survival but couldn't that be superimposed on the earlier declines. There is a cryptic reference to aquaculture and tidal barriers in the conclusion but no evidence is given.

The decline is not put into the context of an overall decline in Atlantic salmon populations throughout their range -- this one is obviously a very bad decline but it is at least somewhat similar to an overall decline everywhere. How does this one compare to other rivers, or to the species as a whole ?

The last three paras under « Special significance of the species » pp 12-13 are not really relevant to the status assessment (nor to the section title) and should not be included.

The « Evaluation » section at the end should be reconsidered in light of the information presented. It is not completely right that « population has declined because of the recent acute depression in marine survival » -- the idea of a recent decline is not very well documented by the information presented, and the great part of the decline has happened over about 80+ years. It does appear to be true that the stock is at lower levels than in the past, and that we should be worried, especially based on the high incidence of « no young » stations and the statement that conservation requirements are not being met (which should be justified a bit more). No evidence for impacts of salmon farming and Bay of Fundy habitat degradation is given, other than that there are a lot of tidal barriers.

Iola Price, Director, Aquaculture and Oceans Science, Ottawa, as written comments on the manuscript, 9 September 1999. The following is a transcription of those comments excluding word changes:

Abstract:
Should the discussion/review of the level of COSEWIC status not take place first- at least a preliminary review? This seems to make the decision a foregone conclusion.

DFO mixed genes from one river to another (as a policy- and brief mention is made of those events in this paper). The duration in time and the magnitude of that mixing should be noted in this paper because it has relevance to aquaculture issues (gene mixing involving aquaculture fish is seen as a threat but mixing of wild stocks has no impact?) This does not make sense.

Introduction:
Describe the role of hatchery production in the past?
Para 3: Baltic stocks are also unique and salmon biologists make this point about every salmon stock.
Para 4: Only 6-24% of rivers assessed- is this enough to make a determination?

Distribution:
Provide references for the colonization of salmon in other parts of the world. The salmon in BC are not a colonized population yet. Don't believe that this is the case for Salmo salar in NZ.

Protection:
Para 3: Where was the second tag recovered from (only the location of one is mentioned).

Population Identity:
Para 3: Regarding the intentional introduction of foreign stocks: Could this be the source of the problem? Discuss and describe. Last sentence: Bodes well for the future: related the case for aquaculture where the escape situation and "interbreeding" is viewed with alarm.

Trends in Abunudance:
Para 1: historical? Give your time/date reference points.

Stock Status:
Is there a seal predation problem? Is there a lack of food? Discuss.
Para 6: Check the spruce budworm spray program records for information on chemical types and amounts into streams. What is the basis for claiming that the deline in age-1+ parr is not atributed to deterioration in water quality? Give data, reasons.
Para 8: If only 24% of rivers were sampled (as a maximum in any given year- but sometimes only 6% were sampled) is this enough on which to base a conclusion?
Para 9: What happened in 1998? Were salmon reared in cages in 1996, 1997?

Habitat:

Para 1: Check the records for information on type and volume in the spray programs. Ditto for agricultural spray programs. There were major bird kills in NB due to pesticide applications- why not an impact on fish?

Para 3: Where do the salmon smolts go after their smolt year?

General Biology:

Para 1: Are these the two recoveries mentioned on page 4: Clarify for the non-salmon biologists COSEWIC membership the recruitment index.

Para 4: What about chemical impacts from agriculture, forestry, detergents, pulp and paper, loss of prey or changes in prey density and species composition. All should be discussed as per COSEWIC report format requirements.

Limiting Factors:

Water temperature? Water levels? Recreational fishing (catch and release caused some moralities). All should be discussed.

Special significance of the species:

Para 2: Are these valid statements for a COSEWIC document?

Para 3: Such as those caused by DFO practices of mixing river stocks as per page 5? Is the outer Bay population proposed for listing too? Last sentence: statement not clear-what does it mean?

Evaluation:

Para 2: First sentence: no data presented in this report to support this contention.